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of preceding stimuli. In this case it might be said that we are not dealing with a specific effect on the nervous system in the way of modification, but with the preservation of the integrity of the nervous system. But this is a distinction without a valid difference. We are concerned with a condition in the nervous system which makes a certain specific reaction possible, and any other stimuli which are essential to putting the nervous system in such condition, or to maintaining it in such condition, through the reaction evoked, must be considered as a part of the stimulus pattern. Our knowledge of the nervous system does not permit us to go beyond this point.

At the present time, I can see no way of distinguishing usefully between instinct and habit. All reactions are definite responses to definite stimulus patterns, and the exact character of the response is determined in every case by the inherited constitution of the organism *and* the stimulus pattern. All reactions are instinctive: all are acquired. If we consider instinct, we find it to be the form and method of habit-formation: if we consider habit, we find it to be the way in which instinct exhibits itself. Practically, we use the term *instinctive reaction* to designate any reaction whose antecedents we do not care, at the time, to inquire into; by *acquired reaction*, on the other hand, we mean those reactions for whose antecedents we intend to give some account. But let us beware of founding a psychology, social, general, or individual, on such a distinction.

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### MUST WE GIVE UP INSTINCTS IN PSYCHOLOGY?

IN a recent number of this JOURNAL (Vol. XVIII, No. 24) there appeared an article entitled "Giving Up Instincts in Psychology" by Mr. Zing Yang Kuo, of the University of California, in which the writer argued not only that instincts have been overworked as explanatory concepts in psychology, but that, as a matter of fact, there are no such things as instincts in human nature. This point of view and many of the considerations urged in behalf of it are interesting and stimulating. There are several points, however, at which the writer's argument appears to me to be loose and open to attack. For example, emphasis is laid on the fact that "there is no general agreement among the students of instincts as to the number and kinds of instincts." That there is such a lack of general agreement among students of instincts no one would deny. But this does not imply the non-existence of instincts; it merely reflects the lack of scientific accuracy and completeness in this field of investigation.

This lack of accuracy and completeness in enumerating and classifying the instincts is doubtless due in part to the relatively short period of time that instincts have been made the objects of scientific study. It may also be due to the specialized and partial points of view from which instincts have been considered. Students in this field have for the most part been concerned with instinctive tendencies as these are implicated in various social processes and institutions. It was inevitable, because of this circumstance, that confusion and differences should arise with reference to the number and kinds of instincts. But this confusion and these differences may be expected to gradually pass away as methods of study become more refined and the points of view from which investigations are made become more objective.

Again, the writer attempts to establish an analogy between the theory of instinct as implying *a priori* relation of the organism to the environment and the theory of innate ideas; and argues that one is as objectionable as the other. He says: "To assume any inborn tendency is to assume *a priori* relation between the organism and stimulating objects; for every behavior is an interaction between the organism and its surrounding objects. Such an assumption is no less objectionable than the theory of innate ideas. As a matter of fact both the theory of instinct and that of innate ideas are based on the same conception; namely, the conception of *a priori* relation of the organism to external objects" (p. 648). Now if we are warranted in speaking of an inborn tendency as implying *a priori* relation between organism and environment in an objectionable sense, a very large part of the structural and functional equipment of the organism must be regarded as involving the same implication. Relations between the organism and the environment in the way of behavior depend on and presuppose the skeletal system, vital organs, receptors and limbs quite as much as they depend on and presuppose tendencies to action, whether innate or acquired. There is a sense of course in which these relations may be said to be *a priori*, namely in the sense that the structures and functions which they bring into operation are fitted in advance of experience to interact with the environment in significant ways. But there is nothing mysterious or miraculous about *a priori* capacities in this sense. They represent the selective influence of the environment on the life stream from which the organism springs. And whatever there is of *a priori* character about an inborn tendency, *i.e.*, an instinct, is to be accounted for in the same way. Innate ideas, however, when they have been ascribed to the mind, have not been thought of as being *a priori* in this sense. Rather they have been referred to some transcendental source which has been set over against and contrasted

with the empirical and biological agencies conditioning our natures. Mr. Kuo's contention that "both the theory of instinct and that of innate ideas are based on the same conception; namely, that of a *a priori* relation of the organism to external objects," overlooks the fact that the *a priori* character of the relation in the two cases is entirely different.

Furthermore, it would seem that while Mr. Kuo rejects the notion of an "inborn tendency" because it implies a *a priori* relation of the organism to the environment, he nevertheless is forced to presuppose this conception in accounting for the development of behavior. More specifically, it is admitted that "the human infant is endowed with a great number of units of reaction" (p. 658); and by "units of reaction" is meant the "elementary acts out of which various co-ordinated activities of later life are organized" (p. 658). Now these "units of reaction" or "elementary acts" must be presumed to involve innate neural tracts making possible just these responses and no others, however simple and undifferentiated in character they may be. We may call these responses "spontaneous" or "random"; but these are relative terms. They do not imply that we regard the responses in question as being accidental or unconditioned. We call such responses spontaneous or random because they do not seem to us to fall into any purposive system. And yet, they serve this purpose at least: they are the stuff out of which, as Mr. Kuo says, "the co-ordinated activities of later life are organized." At any rate, these "units of reaction" or "elementary acts" with which the individual is endowed at birth presuppose neural tracts which can only be described as "inborn tendencies" *i.e.*, tendencies to perform certain definite responses and no others; and as such they imply a *a priori* relation between organism and environment of the same character which Mr. Kuo rejects in the case of instincts. Indeed, these "inborn tendencies" which become overt actions upon the presentation of the appropriate stimuli, are called, in another connection, "non-specific instincts" (p. 658). Whatever we may call them, they differ from instincts, as commonly understood, not in the fact that they are irrelevant to environmental conditions as represented by the stimuli which excite them, whereas instincts involve inborn tendencies which are relevant to the environment; but in the fact that they are not organized into systems serving specific biological ends.

But the argument at another point seems to me to imply the existence of instincts in this more specific and purposive sense. I have in mind the interpretation given Spaulding's experiment on the flight of birds. "That the birds could fly without previous education," says Mr. Kuo "was rather due to the maturity of reaction

system. . . . Given a mature reaction system and given an environmental demand, a definite reaction can fairly be predicted" (p. 653). Now it is important to know just what the writer means to include within such a "reaction system" which is capable in advance of all education, when properly stimulated, of executing an intricate and significant action such as full-fledged flight. He speaks of the particular "reaction system" utilized in the flight of Spaulding's birds as including "wings and other flying mechanisms" (p. 653). What are these other "flying mechanisms"? Do they not include nerve centers and nerve connections? And if so, must these not be thought of as forming and ripening in advance of experiences having to do with flight? To account for their tendency and ability, and the tendency and ability of other mechanisms involved, to execute adequate flying movements by reference to their maturity is beside the mark; the question is, did this state of maturity result from former efforts to fly? If not (as in the case of Spaulding's experiment), it must have developed out of conditions which were present in the organism at birth; in which case, I do not see that the notion of instinct can be excluded from a scientific interpretation of the facts.

It is true, of course, that any action, instinctive or acquired, is conditioned by the presentation of an appropriate stimulus. But the stimulus is not the cause of the action; there is no mechanical equivalence between the stimulus and the response that we are able to make out. It is only a cue for the execution of movements provided for in some "reaction system." And given the stimulus and the "reaction system" we can predict the response to be expected only if we have in mind the purpose served by such mechanisms as are under consideration. We can be certain, for example, that birds kept in small boxes until their wings and other flying mechanisms have matured will fly when there is an environmental demand for this action, because we know in advance that wings and the mechanisms connected therewith are developed for flying. Without this advance information, turning the birds loose in the air with nothing to support them might, for all we could tell, result in any other action as well as in flight. And so it is in the case of any combination of stimulus and "reaction system": our ability to predict a definite response always presupposes an insight into the functional character of the relationship involved. Mere knowledge of the stimulus as a brute fact and of the reaction mechanisms as so many structural entities is not sufficient. This means that the primary condition of significant activity, as well as of spontaneous or random movement, is internal rather than external. Whether we think of this condition in terms of McDougall's "drives" or "springs to action" or in terms of Mr. Kuo's "reaction systems," the emphasis falls on the neural struc-

tures of the organism rather than on environmental factors. And however complex this inner tendency to action is, and however much of its complexity and significance it may owe, at any moment of its history, to the modifying influences of education or training, it presupposes a minimum core or foundation in the inherited structures of the organism without which it could not have had a beginning. The minimum core or foundation thus presupposed, in so far as it is inborn, and in so far as it makes possible significant interactions with the environment, is, it seems to me, on the basis of Mr. Kuo's own argument, deserving of the name "instinct."

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### THE MODIFICATION OF INSTINCT

IN an article on "The Modification of Instinct from the Standpoint of Social Psychology," published in the *Psychological Review*, volume 27, 1920, pp. 247-69, I took the position that instinct could be modified by habits formed prior to the instinct's appearance. As a partial support for this view, I cited the following observation made by C. O. Whitman upon pigeons:

If a bird of one species is hatched and reared by a wholly different species, it is very apt when fully grown to prefer to mate with the species under which it has been reared. For example, a male passenger-pigeon that was reared with ring-doves and had remained with that species was ever ready, when fully grown, to mate with any ring-dove, but could never be induced to mate with one of his own species. I kept him away from ring-doves a whole season, in order to see what could be accomplished in the way of getting him mated finally with his own species, but he would never make any advances to the females, and whenever a ring-dove was seen or heard in the yard he was at once attentive.

Professor H. A. Carr, in editing the Whitman manuscript, also directed attention to the principle involved. Since the publication of my paper, this interpretation has been questioned first by Professor James Leuba in private correspondence and last by Mr. Zing Yang Kuo, in a most interesting paper upon "Giving up Instincts in Psychology," this JOURNAL, Volume 18, 1921, pp. 656-7. The criticism urged is that the behavior of the pigeons so modified was not an instinct but a habit. The passenger-pigeons' choices of mates from their own species are themselves, so it is said, the result of training and association and are not innate. Therefore, so the conclusion runs, we do not have the modification of an instinct, but merely the supplanting of one habit by another.

The proper understanding of much human and animal behavior depends to such an extent upon the principle here involved that I wish to suggest the answer to the problem as I see it: